

October 25, 1951.

Dr. Klaus H. Rothfels
Department of Botany
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Toronto 5, Canada.

Dear Rothfels:

Your ms. (private copy) arrived a few days ago, not long after the original was received by the Editors of Genetics, which is now located in this Department. It will not surprise you that the Editors asked me to review it, but there does not seem to be much point in pretending anonymity. I have asked them, therefore, to return the original for your use in any revisions, while my annotations are made in red on the draft copy you sent me.

Most of the comments should be self-explanatory; a few are of a more general nature, as follows:

You have, of course, formal permission to refer to any of our unpublished work. However, rather than cite it as such, I suggest a citation to the CSH manuscript: Lederberg, J., Lederberg, E.M., Zinder, N.D., and Livaly, E.R., 1951 Recombination analysis of bacterial heredity. Cold Spr. Harb. Symp. 16: In Press. The reference may be completed in proof if the pagination is learned in time. There are very few if any places where this would not be preferable. The reference to Cavalli (p.2) is: Cavalli, L.L. 1950 La sessualita nei batteri. Boll. Ist. Sieroterap. Milano 29:21-29. The same thing might be done for Delamater: ~~without~~ his CSH paper, A new cytological basis for bacterial genetics. Finally, some points of our unpublished work may be covered in my chapter in Genetics in the 20th Century (MacMillan 1951), for which I regret there are no reprints.

I hope you will note my suggested amendment of your acknowledgment, for which I thank you.

Was your abbreviation of "N. & N." intentional? If so, it should be explicit: e.g., Newcombe and Nyholm (N.&N.). If the editors accept this, the same might be done for L. or L. et al.,

The usage of Het is, I think, questionable. It was an unfortunate abbreviation for a hypothetical factor leading to nondisjunction or restitution. The resulting exceptions should, I think, be called heterozygotes, persistent diploids, or unreduced exceptions. Het and the crossover regions a, b, c etc. should be underlined.

Four points of content rather than form:

The discussion of L_m is still somewhat obscure, both as to the hypotheses for its origin and the experimental justification for it. p/8 mentions leucine-inhibited prototrophs, but suggests they are elaborated upon later, which I cannot find. Their occurrence might be emphasized more clearly. Also, what account is taken of $L-L_m^+$ as a possible missing class? Do you have any direct evidence (e.g. from TL crosses) of its occurrence? If you happen to have saved an $L+L_m-$ culture, I should appreciate the opportunity of handling it.

I think there may be some hedging on the revertibility of $M-$. I have never gotten a prototroph from 58-161: Have you? If not, it might be worthwhile saying so.

This may be picayune (except for some of our data), but linearity is really well established by this work only for $Lac-V_1-L-T$, and the inclusion of M in the series is more inferential. Miss Fried has been repeating some of the $S^r \times TL$ crosses in M -supplemented medium that I wrote you about earlier, and can find no evidence that M and Lac are linked. The frequency of "a C.O." is determined by something else (segregation anomaly?). I don't mean to insist on this point which is based on work largely suggested by your paper, but I thought the comment would interest you whether or not you wished to amend the ms.

The latter also applies to the next point of information which relates to the question of p.16. $T-L$ ~~segregants~~ segregants from your TL -cross give markedly abnormal or rather novel linkage behavior, in my one experience. If you can confirm this, I would be delighted. The alteration appears, however, to influence the relative frequency of "a C.O.", rather than the other relationships along the arm. Would you be interested to do a comparable study of TL -supplemented crosses using such a parent? - perhaps this is what you meant by the backcross studies you were tempted to do.

This is about all. To turn to less officious matters, I hope that you did submit a discussion based on this work for the CSH volume. I had conflicting reports from Newcombe and Mrs. Warren, both of whom should have known.

Thanks for pointing out that slip about interference. I remember our talking about it, but am not sure whether it was finally corrected.

We've gotten onto a curious byway on streptomycin resistance. Did you notice that many S^s recombinants in your crosses were unusually mutable to $r S^r$? This seems to be the case and suggests that the mutation from S^s to S^s may occur normally in two steps, the first stage being the highly mutable S^s .

Sincerely,

Joshua Lederberg